SIGMA XI QUARTERLY

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SIGMA XI QUARTERLY

Vol. 28 — Autumn, 1940 — No. 3

YALE UNIVERSITY
NEW HAVEN CONNECTICUT

OFFICE OF THE PRESIDENT

To the Society of the Sigma Xi:

I do not need to say that it is a cause of great pride that the National Headquarters of Sigma Xi should come to Yale. We give you hearty welcome and we promise on behalf of the University whatever co-operation and assistance we can provide for the activities of the Society.

Enthusiasm for scientific investigation is an old Yale tradition and since the days of Benjamin Silliman it has not slackened. There has never been a moment in the history of the country when the need of the qualities which a scientific education produces was greater. As we survey world conditions today, whether abroad or in our own country, whether in the field of international relations or domestic affairs, it is clear that the crux of the problem confronting us is the proper application of science to life.

Sigma Xi has in the past rendered to the nation a noble service in its emphasis upon the values inherent in the scientific training, in the encouragement it has given to young students passing through a critical stage in education, in the prestige which it has brought to the achievement of intellectual distinction in the scientific field. In a period when scientific research touched the vital aspects of national life, as never before, it provided a stimulus to original investigation in the broadest sense, with incalculable results. For these accomplishments of the past all university men and the entire nation must be grateful. We look forward with confidence and appreciation to the continuation and development of the influence of Sigma Xi.

Charles Seymour

EDWARD ELLERY —*Alumni Monthly,* Union Coll

EDWARD ELLERY

A Tribute

by

C. E. McClung

Constructive work successfully done is a reward sufficient in itself; and the plain objective record of developments and accomplishments in the history of Sigma Xi during the last eighteen years is a full recompense to our retiring Secretary for all his efforts in its behalf. He would not, himself, ask for more, but, at the same time, it is good for those who have been served to pause in appreciation of the efforts of the burden bearers. An additional reason for recording personal achievements is that they become part of a movement whose future course may be, indeed must be, determined, in large measure, by such past events. For these reasons, or even without any reason except the pleasure it gives the friends of Edward Ellery to state the satisfaction they have had in his services to Sigma Xi as its National Secretary, it is desired to make here a permanent record of their appreciation of him and his work.

Material evidences of his accomplishments have been regularly published in the archives of the Society. There one may read of the numerical increase in chapters and in members, of lectures promoted, of research grants made and of all the helpful aids to more and better research which have been carried out, in large measure, through the Secretary's office. But only those who have been privileged to work intimately with Dr. Ellery can know the amount and degree of self-sacrificing devotion, of sympathy and good judgment which he has brought to bear upon the execution of plans which seem to develop without effort, so carefully and well have they been formulated and executed. After all, the material evidence of such successful operations is only the visible indication of a more fundamental achievement—the growth of a spirit of altruistic, cooperative effort toward gaining and applying new knowledge to better living.

In so large and loosely knit an organization as Sigma Xi such a spiritual development must stem largely from its leaders and, particularly, from its most active executive officer. Necessary rules, too rigidly applied, can do much to limit the outlook and influence of an organization. Dr. Ellery, although outwardly observant of all regulations, sought always such interpretations as would best preserve human values. His kindness and consideration were inexhaustible, and he lays down the cares of his office, secure in the esteem and affection of a multitude of friends. So long as Sigma Xi can command the services of such devoted officers it will continue to play a worthy part in the development of science and scientists. It is indeed fortunate in being able to retain, for a time longer, Dr. Ellery's participation in its affairs in the less strenuous office of President. His interest the Society will always have.

FACT AND FANCY—THE SOCIAL SIGNIFICANCE OF SCIENCE

Edward Ellery
Union College, Schenectady, New York

Benjamin Franklin was an American, a man of the people. He established the American Philosophical Society to promote so-called "useful knowledge." Benjamin Thompson was an Englishman of noble ancestry. He organized the Royal Institution of London to "teach the application of knowledge to the useful purposes of life." Both of these men, from different levels of human society, were controlled by the desire to promote the usefulness of facts in order to aid the race to a realization of its native right to life and liberty and the pursuit of happiness—a right that is not limited to any class.

Both of these men were experimenters, fact finders, bent on seeking the useful. It was Franklin himself who told his day, "It is impossible to imagine the height to which may be carried in a thousand years the power of man over matter"—certainly not the expression of a mere seeker after useful facts.

The question at once arises, what is a useful fact? To what possible use, for example, in a human family on earth 400,000,000 miles away from it could be put the knowledge that Jupiter has five satellites, each with its individual orbit and periodical eclipse? That is certainly a useless fact as far as we in 1940 are concerned. Yet from the observation of those eclipses, Roemer in 1690 fixed the velocity of light, the relation of that velocity to the velocity of other radiations, confirmed by Maxwell's famous equation which led in time to the electro-magnetic waves of Hertz, eventually to Marconi, then the radio, and now to television—useful facts certainly, these latter, stemming from the useless fact of the eclipses of the satellites of a distant planet.

What is a useful fact? Of what possible use in his day could be Faraday's fact of electro-magnetic induction? In 1940 electro-magnetic induction is indispensable. Out of a hole in the wall of any adequately wired house comes heat, cold, light, groceries, entertainment, the voice, and soon the vision, of our friends, the policeman, the doctor, the minister. Upon Faraday's useless fact of electro-magnetic induction has been built an electrically driven human society "that does not run when the power is turned off."

What is a useful fact? Of what possible use in a world of men like us could a gas be discovered by the useless spectroscope in minute traces on the surface of the sun 93,000,000 miles away? Later, isolated in inaccessible parts of the earth itself, helium's dollar value was about $2,000,000 a cubic foot. Now extracted from natural gas at a cost of about 1c a cubic foot, increasingly useful because of its extreme lightness and non-inflammability, and perhaps in a few years indispensable as an ingredient of those oxygen mixtures which form a breathing atmosphere for victims of respiratory diseases.

What is a useful fact? There is no definition. Facts can no longer be classified as useful and useless. We so-called moderns know that. No man can prophesy the future of a fact when it has once been indisputably established, because the mind's fancies cannot be reduced to a formula. The scientist is more

than the fact finder, research more than organized fact finding. The scientist has never been the dreamless fact finder. The scientist has never been exclusively the cold-hearted unimaginative intelligence, also never exclusively the warm-hearted unpractical human being. Franklin and Thompson were shrewd theorizers, scientific prophets, men of many fancies. The scientist has always been both intelligent and imaginative. Therein lies the social significance of science, the invaluable importance and influence of the scientists as a social group.

Facts without dreams are futile. Dreams without facts are dangerous. Men act according to their beliefs, and if their beliefs are illusions, the effects of their acts are all too often tragic. There are men in human society who still deliberately avoid or discard facts. They are always a social menace. As Voltaire is said to have remarked once, "As long as people believe absurdities, they will commit atrocities." Real progress is made only as beliefs are true. The scientist dispels illusions, gets the facts. Fact finding and dreaming, an indispensable, unconquerable union which removes the dangers of guesses, renders inevitable effective progress of human civilization—fact finding and dreaming, "useless each without the other."

In every social problem as well as in scientific research, we dare not shut our eyes to truth, even the most blazing. No reasonable man turns his gaze from the plain facts of contemporary life. He has no desire to escape the realities of life. To do that is to enter upon a fool's paradise. The true scientist who knows what is and what ought to be is the safe guide for a confused humanity.

It is difficult for us in 1940, recognizing the inestimable value of a union of fact finding and dreaming, to realize that the struggle between the fact finder, knowing indisputable truth, and the dreamer whose incentive is a myth, persisted so long, has not yet passed. So accustomed are we of 1940 to rapid movement, so impatient at delays, it is difficult for us to sense what the struggle has involved. Through many centuries men failed to realize that fact finding was the most dependable and useful factor in the advancement of the race. Aristotle for a hundred years and more was an intellectual model for thinking man. But Aristotle and his followers and imitators had more use for logic than for observation of the world they lived in. St. Ambrose (389 A. D.) wrote, "To discuss the nature of the earth does not help us in our hope of the life to come. It is enough to know that scripture states that He hung up the earth on nothing. Why argue whether He hung it up in air or on water? The majesty of God constrains it by the law of His will." That was the concept that maintained a strangle hold on the mind of man until Roger Bacon made his first scientific experiments. But even then centuries passed before men were willing to accept facts. Copernicus was a heretic when he showed that the earth revolved around the sun. All the experimental scientists, the fact finders of those early years of experimental science, were heretics. Newton was an irresistible force, but the theologians did not surrender. Priestly was a heretic. A brewery was certainly a queer place for a minister, but in a brewery Priestly expended exhaustless energy, displayed his indomitable spirit, while he was getting the facts about the gaseous product of the beer's fermentation. The press of England, the Church, Parliament, even the scientists of the day made England unbearable for Priestly. That was as late as the early 1800's. So difficult was it for the human mind to realize the unspeakable, priceless value of a fact, and the immeasurable

treasure of thought freedom. It was the Tammany Society of New York (of all organizations), described in those days as "a numerous body of freemen who associate to cultivate among them the love of liberty," who welcomed the fact finder Priestly to the United States as the man "who had fled from the rude menace of violence, from the flames of bigotry, from the rod of lawless power, to the refuge of freedom and of peace among the Americans."

Even in America, where we glory in freedom, are jealous of what we call our heritage of freedom, resist its curtailment with all the power we possess; even in America, this fact-finding process developed with incredible slowness. For hundreds of years in America our educational institutions were controlled by the Church, or, if not by the Church, by the prejudices of classicists "who had simply changed control from theological into literary revelation." What Newton had found so uncongenial and so deadening in the atmosphere of Cambridge University was characteristic of our American universities even up to the 1800's. Even Andrew White, president of Cornell, tells in his autobiography of the disgust he felt when he saw a student in the Sheffield Scientific School examining a colored liquid in a test tube, "disgust that any human being should give his time to pursuits so futile." That was the attitude that was cultivated by the professors in their students in many American colleges and universities even close up to our own day.

It may not be out of place for me to refer to the fact that Union College was among the first to shake off that strangle hold which theology and literary methods had upon American universities and colleges.[1] As early as 1798, three years after Union College was founded, the then trustees bought experimental apparatus for a laboratory in physics. In 1823 science courses were introduced. In 1827 the classics gave way to the modern languages. In the early sixties Professor Joy was sent to Europe to buy apparatus for the chemical laboratory. It is not surprising that Francis Whelen, a graduate of Union in 1813, who became president of Brown in 1827, and Henry Tappan, a graduate of Union in 1925 and who became president of the University of Michigan in 1852, educated in añ atmosphere which encouraged the union of fact and dream as exemplified in courses of science, should have carried to the two institutions which they served as president this inestimable value of fact finding, and spread the idea among their students, their trustees, and the larger public of which the institutions were the nucleus. The union of scientific and utilitarian education with the dreams of the workers have added to life more values than can be measured by any unit man knows.

A striking illustration of the recognition of the value of combining search for facts with desire for what is not, is in the awards of the National Association of Manufacturers during the last week of February. Five hundred living research workers and inventors were recognized with suitable ceremony. "The ingenious mechanic was notably absent from the list. He has passed forever with the rise of the theorist, the pure scientist, the unpractical research worker in the industrial and the educational research laboratories, and the results have been phenomenal." The prizes awarded by the National Association of Manufacturers were won, for the most part, by the fact finders, the research scientists

[1] See "Two Gentlemen of Union" by President Cowley, *Sigma Xi Quarterly*, Summer, 1940.

In the New York area alone, 68 percent were physicists, chemists, and technologists, directors of industrial research laboratories, university professors. The solution of industrial problems of the modern day, the way to improvement of the old product of industry to the new that makes physical life easier and more comfortable, demands the abilities of the mathematician, the research scientist, the fact finders, and, just as much, the men who dream of what is not and strongly desire it. If progress is to be made, fact finding and dreaming must be inseparable. "Industry cannot wait for something to turn up. Industry now turns something up!"

So much is this the twentieth century method that the search for facts has become an industry in itself. Industrial research laboratories now spend $200,000,000 a year, and employ 50,000 trained workers. Even the small industries turn to private consulting laboratories, or such organizations as the Mellon Institute, and for a few thousand dollars expand along new technological lines, reducing to a minimum infection dangers by conditioning, not simply washing, dishes; clarifying knowledge of the proteins as centers of many life activities; building gas and liquid meters out of synthetic resins, and plastics, etc. Modern industry knows that it must persistently and relentlessly bring about the realization of something that is not, something that someone has dreamed about. Otherwise industry will perish.

The laboratory of the experimental scientist is sometimes and falsely thought of as a cloister, effectively isolated from human life; the research scientist, a monk living and working exclusively in a world of material and physical facts. And yet in no single group of men anywhere in this confused world is to be found a more striking exhibition of the value of combining the dream with the fact—the value of unity in the midst of differences—than among the scientists; a unity in strong impressive contrast with the frictions of groups against groups, race against race, class against class. Here is something for all men to contemplate.

One hundred thousand research workers all over the world, devoting their lives to bringing their dreams to reality, with no thought of personal benefit, interested more in facts and dreams than in fame, publishing results freely from which humanity may eventually benefit, meeting frequently by thousands in national and international congresses, with friendly, open-minded free discussion, without racial or religious friction—there is a spectacle for the pessimist who thinks that strife is a biological necessity based on a survival-of-the-fittest principle. Thousands of men and women sinking differences in a common cause—that means something. No thinking man can dismiss the phenomenon when weighing human relations. Thoughtful men everywhere are profoundly impressed by the spiritual example a brotherhood of scientists sets.

There is no compulsion or regimentation in scientific congresses. They are the result of determination to cooperate. Good faith, integrity, responsibility, desire to cooperate—without these, social, economic and labor laws are useless so far as permanent accomplishments are concerned, and these are the very qualities which the group of scientists so impressively exhibits, and which are essential to all science progress.

Brotherhood—how far we are today from the realization of the great idea of the brotherhood of man, the so-called permanent myth of mankind, several thousand years old! It has never been realized, so men say. One hundred thousand and more scientists, with unmistakable unity of impulse and interests, convincingly refute that pessimism.

The social significance of science! How often we have read or heard, "Blessed are the meek, for they shall inherit the earth," and how often have we questioned and doubted the truth of it, have called it a mere fantasy, a Utopian ideal not realizable in the human family! The meek do not inherit the earth. But, as Dr. Fosdick points out, if the world today would look at the realm of science and the great groups of scientific workers, it would find a striking illustration of the great truth that the meek shall inherit the earth. Who are the meek? They are the human individuals who realize that they do not know it all, who recognize the limitations of their own knowledge, who are always seeking new knowledge, are always humble in the realization of the vast amount they do not know. That is the true scientist. The true scientist gives the greatest exhibition of true humility. He is always teachable. In the realm of science no one except the teachable has the slightest chance of advancing knowledge in his field. "The scientist is the new kind of conqueror, who has been re-making the earth for 300 years and more, and will be re-making it long after the proud, the violent, the dictator, the autocrat have fallen." It is these teachable scientists who know some facts, dream what they think ought to be on the basis of the facts they know, not contented with themselves, their knowledge and the world they live in, the disbelievers because they know some facts—the true scientists, the humble, the teachable, the meek—who have actually inherited the earth.

Fact and fancy—both essential to progress; neither alone sufficient. The scientist and his work present before the world at large a phenomenon of profound social significance. The true scientist knows and shows that "man's reach must exceed his grasp"—that is "what heaven is for."

SIGMA XI NATIONAL LECTURESHIPS—1941

Dr. James Franck—Department of Chemistry, University of Chicago, Chicago, Ill.

"Fundamentals of Photosynthesis"

Photosynthesis in green plants, the only available source of food on earth, is a complicated photochemical process taking place under the influence of sunlight. A survey of the most important facts will be given and their most probable physical interpretations will be discussed. Especial attention will be paid to the phenomena of the light situation, the induction periods of photosynthesis, and the fluorescence of living leaves.

Dr. Perrin H. Long—The Johns Hopkins Hospital, Baltimore, Md.

"Recent Advances in Bacterial Chemotherapy, with Special Reference to the Mode of Action of Sulfanilamide and Its Derivatives"

Intense interest has been aroused by the use of sulfanilamide and its derivatives in the treatment of bacterial infections. However, up until the present time, the manner in which these chemical compounds act upon the infecting bacteria is not well understood. There are three main theories concerning the mode of action of sulfanilamide: (1) The oxidation and reduction theory; (2) The anticatalase theory; (3) The peptone inhibition theory. These theories will be discussed and subjected to critical analysis, and new and original information bearing upon this subject will be presented.

Dr. I. I. Rabi—Department of Physics, Columbia University, New York City

"Radio Frequency Spectra and the Magnetic Properties of Atomic Nuclei"

Atoms and molecules in their free states emit radiation not only in the visible and infra red regions, but also in the long wave length region which corresponds to the radio waves of common use. By the use of atomic and molecular beams in high vacuum under collision-free conditions, the atoms are made to absorb and are stimulated to emit these radiations by subjecting them to the influence of oscillating electromagnetic fields of radio frequency. The consequent changes in the individual atoms are detected through the alteration of their magnetic properties. This new field of physical investigation, which has hitherto been inaccessible, is now open. The methods have been applied chiefly to the study of nuclear properties such as spin, magnetic movement, and electrical quadrupole moment. The results of these experiments will be described and also their bearing on the nature of nuclear forces and nuclear structure.

Dr. Harlow Shapley—Harvard College Observatory, Cambridge, Mass.

"In Defense of the Universe"

Professor Shapley's research activities have been related to the studies of galaxies and the problems of the Metagalaxy. His lecture will include a report on some of the thrilling portions of these investigations, especially in the star clouds of Magellan, which are less than a hundred thousand light years distant; also on the anatomy of these somewhat disordered universes. He will show how from the Magellanic Clouds some of the most important astronomical tools have been obtained. The lecture will explain how a census is taken of the million galaxies that are within about a hundred million light years of the observer, and how, through the study of the distribution of these galaxies, such problems as the age of the universe, the red shift, the mass of population of galaxies, and the structure and meaning of clusters of galaxies are being worked out.

Wherever a 35 mm. moving picture projection apparatus is available, Doctor Shapley will show marvelous moving pictures of the storms on the surface of the sun as photographed from Pic du Midi by M. Bernard Lyot, the young French inventor of the coronagraph.

Dr. V. K. Zworykin—RCA Electronic Research Lab., Camden, N. J.

"Image Formation by Electrons"

Electrons moving in potential fields are shown to follow trajectories which are mathematically identical to light rays in refractive media. This forms the basis of the branch of electronics known as "electron optics." Electron optics finds important application in modern television. The design of the electron guns used in the Kinescope and the Iconoscope is considered, together with the process of image formation in a television system. Similarly, electron optics has proved of immense value when applied to the problem of obtaining very high useful magnifications. Several forms of electron microscope are described in some detail, special attention being given to the reasons for their superior resolving power as compared with that of a light microscope. Results already obtained with these instruments show them to be the greatest advance in the field of microscopy since the invention of the compound microscope.

AWARDS OF SIGMA XI GRANTS-IN-AID OF RESEARCH—1940-41

At the annual meeting of the Committee on Award of Grants-in-Aid of Research, held in Cambridge, Mass., July 22, 1940, the following grants were made for the academic year 1940-41. See pages 134-139 for the list of contributors.

F. T. Addicott, Santa Barbara State College. $400 for studies on the action of hormones in the growth and propagation of plants.

R. W. Fautin, University of Illinois. $300 for ecological investigations of the biotic communities of the Great Basin Desert in west central Utah.

R. A. Fennell, Michigan State College. $50 for studies on nutrition of Amoeba.

A. C. Giese, Stanford University. $100 for studies of effects of ultraviolet light on respiration of micro-organisms.

H. M. Huffman, California Institute of Technology. $400 for thermochemical investigations of physiologically important substances; determination of heat capacities over a wide temperature range; the determination of heats of combustion.

A. V. Hunninen, Oklahoma City University. $100 for work on the life cycle of Trematodes and on the dwarf tapeworm.

F. R. Kille, Swarthmore College. $300 for histological studies of the origin of the germ cells and the seasonal variations in the reproductive system of the holothurians.

L. H. Kleinholz, Harvard University. $100 for continuation of studies on distribution and physiology of intermedin.

W. J. Luyten, University of Minnesota. $150 for aid in the publication of research on "The Stream and Solar Motions of 94,000 Stars in the Southern Hemisphere."

E. P. Mumford, Stanford University. $500 for studies of animal and plant distribution in oceanic islands.

J. V. Neel, Dartmouth College. $100 for an investigation on the relation between fly size and mutant expression in Drosophila.

T. Russell Wilkins, University of Rochester. $100 for studies on the half life of Uranium 235 by a new method depending on observations of the range of alpha particles in thick emulsions.

Mary J. Willard, Pennsylvania State College. $250 for research on the optical properties of the nitrophthalimides.

Dorothy M. Wrinch, Johns Hopkins University. $500 for research on the structure of biologically active protein molecules, with special reference to the structure of insulin.

THE UNITY OF THE SCIENCES OF THE SEA [1]

H. U. SVERDRUP

Scripps Institution of Oceanography, University of California
La Jolla, Calif.

Anyone who examines a list of publications from an oceanographic institution will notice the diversity of the subjects dealt with. Let me quote a brief series of titles of such papers published in recent years:

On the mutual adjustment of pressure and velocity distributions in certain simple current systems.

Periodic changes in spectral scattering and spectral transmission of daylight in tidal water.

The relation of dissolved oxygen to nitrogen in some Atlantic waters.

Concerning the copepod, *Eucalanus elongatus* Dana, and its varieties in the northeast Pacific.

The biological approach to the preparation of anti-fouling paints.

Radium content of some inshore bottom samples in the Pacific Northwest.

This enumeration could be continued indefinitely and a more complete list of titles would leave the impression that oceanography is nothing but a wide tarpaulin spread over an exhibit of products which are labelled "marine," or that it is a bag into which are packed all subjects more or less related to the character of the ocean waters, the organisms living in the sea, the features of the ocean bottom, and those dealing with the effect of the oceans on climate and the commercial utilization of sea products.

Superficially, there seems to be no unity within the marine sciences, but the obvious reasons for this apparent conglomeration of unrelated subjects are that our comprehension has to increase step by step and that each little addition to our knowledge has to be made within a special field. Oceanography may sometime in the distant future give us a many-colored picture of the oceans, but at the present time we have only started working with a large and intriguing puzzle game, each one of us trying to put together pieces of similar color, hoping that some time all fragments can be joined into one complete picture. It is as yet far too early to guess what this picture will look like, but it is not difficult to visualize oceanography as a unified field of research.

The oceans can be considered an immense aquarium in which numerous kinds of organisms live, bacteria, microscopic plants, giant kelps, tiny primitive animals, and the largest mammals known ever to have existed on earth. In this balanced aquarium plants synthesize organic matter in the presence of light, and other forms live off the plants as is the case on land. The conditions for plant growth are therefore of the utmost importance to all of the organisms in the oceans. These conditions vary from one part of the sea to another but, generalizing, it can be stated that the ocean waters represent a weak solution of nutrient salts in which peculiarly adapted plants can thrive. The cultivation of plants without soil, experiments in hydroponics, are now attracting wide attention, and

[1] "Contributions from the Scripps Institution of Oceanography, New Series, No. 113."

it may be well to remember that in the ocean plants have grown without soil as far back as the geological history of the earth goes.

The occurrence of plants is limited to the upper layers of the oceans where solar energy penetrates in sufficient quantity to make photosynthesis possible. In the open ocean the plants must float freely and must have relatively large surfaces to be able to absorb sufficient quantities of phosphates, nitrates, and other mineral salts which are dissolved in the sea water and which the plants need besides light. Thus, in the photosynthetic zone, plant nutrients are extracted from the water, but simultaneously such nutrient salts are brought back in solution by the mineralization of the tissues of decomposing plants and animals. However, some organic remains sink below the photosynthetic zone before they are decomposed and part of the essential elements are removed from the surface layers, therefore the amounts available for plant growth would be exhausted if there were no mechanism for replenishment. Such a mechanism exists, because in the deeper water where no plants extract the nutrients a storage supply is maintained from which replenishment of the nutrients in the surface layers, or what one may call fertilization of the ocean pastures, is provided wherever subsurface water is brought to the surface either by ascending motion or by processes of mixing which extend to great depths. Large-scale ascending motion of subsurface water takes place along the west coast of the greater part of the American continents, along the west coast of South and North Africa, and to a more limited extent in other regions. Vertical mixing reaching to great depths occurs in high latitudes, and the areas here mentioned are the best ocean pastures, showing large populations of organisms in contrast to the open ocean regions in middle latitudes. It is characteristic that the great commercial fisheries exist in localities where relatively rapid replenishment of the plant nutrients of the surface layers takes place.

If the ocean is compared to an enormous aquarium, oceanography represents the science dealing with the functioning of this aquarium. The physical oceanographer has to account for the ocean currents which maintain an exchange of water between the different parts of the sea and he has to examine the slow ascending motions and the processes of mixing by means of which plant nutrients are brought to the surface layers. These studies correspond to studies of the stirring of the aquarium waters which provide for the fertilization of the top layers. The physical oceanographer also has to account for the mechanism by which the ocean waters are heated or cooled and by which the average temperature in different regions is kept constant, or, in other words, he has to provide information as to the thermal characteristics of the aquarium. He has to examine the penetration of light in the sea and to establish what wave lengths and what amounts of radiation reach the different depths in the different parts of the ocean. When dealing with these and similar questions the physical oceanographer need not be concerned with the populations of the ocean, but he should not forget that his work is part of a large-scale approach to an understanding of all events in the sea.

The chemist can furnish data which greatly help in the study of the origin and distribution of the water masses and the character of the currents, and data which have bearing upon the characteristics of the ocean waters as a weak solution of

nutrient salts. He faces a number of difficulties because sea water is a highly complex solution in which some salts are present in relatively high concentrations and others in minute quantities, and the study of the chemistry of sea water therefore requires special skill and accuracy, regardless of how results are applied. During the early history of oceanography emphasis was placed on the type of chemical knowledge which was applicable to problems in physical oceanography, but lately those which have bearing on biological problems are advancing more and more to the foreground.

The biologist is confronted with an unlimited number of problems because the biological sciences will naturally occupy the center of the field when the aquarium picture is used. The numerous kinds of organisms inhabiting the sea have to be known—from bacteria to whales. Their reactions to changing environments, their feeding habits, and methods of reproduction have to be examined. Studies of the complicated life histories characteristic of many marine organisms are essential in order to recognize the early developmental stages of common forms. I may remind you of one of the most dramatic episodes in oceanography, the hunt for the spawning grounds of the Atlantic eel, which started when a transparent flat creature was recognized as the larval form of the eel and which ended when it was shown that all these larvae drifted out from the Sargasso Sea to which the mature eels had migrated in order to spawn and die.

When the sea is considered as a big aquarium the ecology must receive great attention. In many cases one can deal with groups of organisms without emphasis on the individuals and without consideration of species represented in the populations. The interrelations of these groups become matters of major importance owing to the destructive competition for food which is met with everywhere in the sea.

In most marine biological problems knowledge derived from work in physical and chemical oceanography is indispensable. This is particularly true when dealing with ecological questions which cannot be answered without consideration of the environment of the organisms. I am here using the term environment in a broad sense, including the temperature of the water, the total salt content, the state of motion, the amounts of plant nutrients, the oxygen and the carbon-dioxide content of the water, the presence of other organisms than the group under consideration, and many other factors.

The geologist has to deal with the bottom of the aquarium and to study problems relating to the types of sediments found in different regions and the processes of sedimentation. He must be familiar with the mechanism by which sediments can be transported, he must know the sources of different sedimentary material, and must be acquainted with the early transformation of sediments by the activity of organisms. The study of sediments therefore requires knowledge of the state of motion of the water and of the type of organisms whose skeletal structures contribute to the formation of the bottom deposits, and it also necessitates knowledge of the chemistry of sea water and the conditions favoring solution or precipitation of such substances as calcium carbonate and silica.

Thus, the picture of the oceans as a big aquarium unites all the different branches of oceanography under a common point of view and makes evident the interrelation between the apparently separate marine sciences.

One may now ask if the unity of the sciences of the sea stops here. Do we who are engaged in studies of special functions of this aquarium have nothing in common but our distant goal? Or is it possible to point out methods of attack which are similar within the different branches so that we have not only a common goal but are also advancing along similar lines? My answer to these questions is that a number of the methods of approach are similar and that the unity of the sciences of the sea is much greater than may appear from the comparison with an aquarium.

In the first place, it should be stressed that the marine sciences essentially deal with conditions in nature and have, therefore, to be based upon observations of events in nature and not upon laboratory experiments or theoretical considerations. Controlled experiments and theoretical researches are necessary, but it should always be borne in mind that such studies are auxiliary and should be planned to facilitate the understanding of what is observed or may be observed in nature. Experiments and theories are tools without which we cannot expect to disentangle the complicated events in nature and arrange them in logical sequence. We should be helpless without tools but we would not study oceanography if data from the sea were lacking. Every oceanographic laboratory has to face the sea and every oceanographer has to go to sea!

Turning to the sea, let me draw attention to the fact that in all branches of oceanography the concept of an average steady state has found wide application. This concept is based on the experience that observations of conditions in the ocean conducted over a long period of time and in a large enough space do not show a continuous change, but demonstrate that, on an average, time changes in any given locality disappear. Superimposed upon such a steady state are periodic fluctuations related to the changes from day to night, or to seasonal changes, and superimposed are also what we call random fluctuations which, however, never continue to alter conditions in the same direction during a long time-interval, but always revert towards the average state. I shall return to the matter of periodic and random fluctuations, but wish first to discuss the importance of the concept of a steady state.

Detailed knowledge of the average steady state is in itself not important because the ultimate goal of oceanography is the same as that of all natural sciences: to predict coming events, and prediction has to be based on knowledge of processes and not on a description of unchanging conditions. However, we are not dealing with static conditions but with a delicate dynamic equilibrium which is maintained by opposing factors, and we are studying the steady state in order to gain knowledge of these factors. If some of them can be observed, others may be determined indirectly because we know that on an average their effect must balance the effect of the observed ones. Thus, the concept of a steady state receives its importance when one realizes that it implies the concept of a state of dynamic equilibrium.

The application of this principle of dynamic equilibrium can be illustrated by a number of examples. Consider first the heat budget of the oceans. This heat budget is exactly balanced, meaning that the amount of heat received by the oceans exactly equals the loss of heat from the oceans. The amounts of heat conducted from the interior of the earth can be disregarded because it is negligible

compared to that received by absorpton of short-wave radiation from the sun and the sky and by absorption of long-wave radiation from the atmosphere, or lost by long-wave back radiation. All processes of radiation combined lead to a gain of heat which can be measured with considerable accuracy, and which must equal the loss by conduction of sensible heat to the atmosphere and by evaporation. The amount which is lost as sensible heat can be approximately estimated and, thus, a foundation exists for computing the average amount of evaporation from the surfaces of the ocean. Such computations based entirely on the concept of heat balance have led to the conclusions that the average evaporation from all oceans amounts to very nearly 100 cm. per year (39 inches). This result represents a valuable check on directly measured values which have to be subjected to a number of uncertain corrections.

As another example of the application of the same principle, let us consider the currents through the Strait of Gibraltar which connects the Mediterranean Sea with the open Atlantic Ocean. In the Mediterranean Sea evaporation greatly exceeds precipitation and runoff, and there must, therefore, be a net inflow of Atlantic water into the Mediterranean Sea in order to compensate for the water lost by excess evaporation. On the other hand, the net salt transport through the Strait of Gibraltar must be zero because, otherwise, the salinity of the Mediterranean waters would decrease or increase. Now, the Atlantic water which flows in is of high salinity, about 36.2 per mille. If the net transport of salt shall be nil Mediterranean water of even higher salinity must flow out along the bottom, but the amount of water flowing out must be smaller because a fraction of the inflowing Atlantic water has evaporated. Observations in the Strait of Gibraltar indicate that this is exactly what happens, but the data are incomplete, for which reason the concept of a dynamic equilibrium greatly helps the interpretation of the observations and the understanding of the processes which operate. On an average, about 36.3 cubic miles of Atlantic water flow into the Mediterranean every 24 hours and 34.9 cubic miles of Mediterranean water flow out. The difference between these amounts, about 1.4 cubic miles per day, represents the excess of evaporation over precipitation and runoff.

So far, my examples have been taken from the field of physical oceanography, but in order to support my contention that the same principle is generally applicable, I have to present examples from other fields. Let us, therefore, examine the distribution of oxygen in the ocean. The oxygen which is present in the sea has either been absorbed from the air at the sea surface or it has been produced in the surface layers by the photosynthetic processes of plants. In the surface layers the water is generally saturated with oxygen but at greater depths lower oxygen values are found, because there oxygen is being removed from the water by the respiration of organisms or is utilized in some other manner for oxidation of organic matter. Studying the distribution of oxygen in the deep water one finds, however, that it varies widely from one locality to another, and a minimum oxygen layer which is found at an intermediate depth in most oceans has attracted wide attention. When discussing the general distribution of oxygen it should again be borne in mind that, on an average, a state of dynamic equilibrium exists, meaning that if observations are repeated one will always find nearly the same amount of oxygen present at a given point. Certain seasonal variations may be found but

averages based on observations in different seasons and different years will not show any tendency to change in a constant direction. At any point replenishment of oxygen takes place by processes of mixing or by horizontal currents, and consumption of oxygen takes place by oxidation of organic matter. On an average, replenishment must exactly balance consumption because, if that were not the case, the amount of oxygen in the locality under consideration would either decrease or increase. On the basis of this principle of dynamic equilibrium some idea has been obtained of the actual oxygen consumption within the different parts of the ocean. This has not been measured by studying the biological processes which take place but by computing the replenishment on the basis of knowledge of mixing and currents, and the values for the replenishment have been considered representative of the consumption which, in the examined regions, has been found to be of the order of one milliliter of oxygen per liter of sea water per year.

Let us examine still another question. It has been found that the ocean waters, in general, contain small amounts of dissolved organic matter. The amounts are so small that their presence is best demonstrated by the fact that sea water collected from any depth is capable of furnishing nourishment for a population of heterotrophic bacteria if such conditions are created that the bacteria can develop. The procedure used in these determinations is to saturate a number of water samples from a certain depth with oxygen and determine the amount of oxygen consumed by the bacteria. It has repeatedly been found that the populations of bacteria will increase to a certain limit and will consume a definite amount of oxygen. It is assumed that the oxygen thus consumed is used for oxidation of part of the organic matter which was originally present in the water, and from the total amount of oxygen consumed one can compute how much of the organic matter in the sea water has been utilized by the bacteria. The amounts determined in this manner are of the order of 5 milligrams per liter of water, meaning that the concentration of the organic matter is at least five parts in one million.

On the basis of these findings certain speculations can be advanced. Laboratory experiments show that bacteria can reduce the concentration of organic matter in sea water to values as low as one part in one hundred millions. Why, then, do not the bacteria in the sea reduce the concentration existing there below five parts in one million? The answer appears to be that when a sample of sea water is enclosed in a bottle organic matter is absorbed by the surface of the bottle and thereby becomes available to the bacteria adhering to these surfaces, whereas in the deep ocean where no solid surfaces exist the bacteria cannot utilize all the organic matter present. But why should the limiting concentration be about five parts in a million and not fifty parts in a million or one part in ten millions? The answer is perhaps that we have again a state of dynamic equilibrium at which the replenishment of dissolved organic matter equals the consumption by bacteria, the latter being partly controlled by existence or non-existence of solid surfaces to which the bacteria can adhere. In this case none of the factors which maintain the balance are known, but the principle of dynamic equilibrium helps towards clarifying the problems involved and towards developing methods of attacking these problems.

This principle of a dynamic equilibrium can also be applied on a large scale to the different types of populations in the sea. The average quantity of phytoplankton, of the different forms of zooplankton, and even of the different species of fishes and of the large mammals of the sea, remains unaltered owing to the competition between the different groups. It is true that the existing balance is occasionally seriously interfered with by the activity of man, but over a long period of time his activity will be governed by similar laws of competition because his efforts towards utilizing to the utmost certain species of fishes or such mammals as the whale, will depend upon his economic return. When the fish becomes scarce his efforts will diminish and a new state of equilibrium will be reached which is characterized only by the fact that man has joined the group of aquatic beasts of prey.

I may add that the sea is far from being a régime of peaceful negotiations. To the contrary, it is difficult to visualize any region in which there is more cut-throat competition between species than there exists in the ocean, or a region in which a balance between species is maintained by more rigorous means. One has to consider not only the competition between the different species but also the difficulties encountered by the organisms of the sea owing to the very character of the environment. I may mention the fact that many marine organisms have no means of locomotion but are, throughout their life-span, carried by ocean currents and are unable to secure for themselves more suitable environmental conditions, and that many organisms during some stage of development are similarly transported helplessly by the moving water masses. No wonder that most marine organisms are prolific in their production of offspring because few have a chance to survive and reach maturity!

I have pointed out that the concept of a dynamic equilibrium is helpful in understanding the aquarium with which we deal, and that this concept can be used successfully when applied to a large number of oceanographic problems. However, such a principle is by no means characteristic of the marine sciences only, because it is useful in many other instances as well. It has found application to ecological problems in zoology and botany, in climatology and other natural sciences, and even in the field of social science. There exists, however, another principle which finds its widest application within oceanography, that which is based on the fact that the waters of the ocean represent a continuous medium, for which reason all events in the sea must follow the laws characteristic of a continuous medium.

When dealing with the dynamic equilibrium, emphasis is placed exclusively on what happens in a given locality, and the definition of a steady state is that time-changes in a given locality are zero. In the ocean it is necessary, however, to distinguish between what happens in a given locality and what happens within an individual water mass which is in constant motion from one locality to another. When dealing with the heat budget of the ocean it was stated that on an average the gains and losses of heat are equal, but this applies to the ocean as a whole, and the heat content of an individual water mass may be subject to great changes. In physical oceanography one distinguishes between the local and the individual changes, and such distinctions will be helpful in many other cases and will prevent erroneous conclusions. Let me take an example from the field of sedimenta-

tion. On a wide continental shelf the coarsest sediments are found near the coast and near the outer border of the shelf, whereas finer sediments are found between these regions. It may be concluded that the finer sediments are being deposited on the middle part of the shelf, but the observed distribution can exist even if no net deposition of sediments takes place anywhere on the shelf. There may be a steady transport of finer sediments from the regions near the coast and across the border of the shelf. Finer particles brought from the coast may be deposited at some distance offshore, but in the localities where they are deposited other particles are picked up by moving eddies and carried farther out. These are deposited somewhere else and new particles are picked up, and so on until, finally, an amount passes the outer border of the shelf equal to the amount which was added near shore. Thus we have a steady state without any net deposition, but each individual particle is more or less on the move, and where it will come to rest cannot be predicted.

When dealing with biological phenomena it is particularly important to be aware of the difference between local and individual changes. Observations of phytoplankton at a fixed locality often indicate sudden changes in the amount of phytoplankton present. It may be concluded that the rate of reproduction of the phytoplankton has suddenly changed or that the rate of grazing has suddenly increased, but such conclusions are invalid unless it can be shown that no new water-mass has entered the area. One has to remember that the water is in motion, that sharp transitions often exist between different types of water and that local changes may be due to intrusion of a new water-mass and not to the operation of local factors.

The concept of the continuity of the ocean waters and the consequences of that continuity has only in part found application to biological problems, but I am convinced that it will enter more into the foreground and that it will become a powerful tool characteristic of all the marine sciences.

When leaving the examination of the average steady state we have to realize, in the first place, that changes may be caused in part by agencies which operate locally and, in párt, by factors which were in operation somewhere else but are felt in our locality owing to the state of motion of the water. Let me take a somewhat extreme example. During the past winter considerable damage was done along beaches in the Los Angeles district, particularly at Santa Monica and Redondo Beach, where numerous structures were destroyed by exceptionally large waves. The destruction caused considerable excitement, and in the press it was suggested that these destructive waves were due to some "mysterious submarine force." However, the reported waves were surface waves of periods often observed in the region but of unusual height, and they occurred on each occasion after a severe storm in the North Pacific. I do not doubt for a moment that they were ordinary swells created by strong winds at sea which, owing to the configuration of the coast, were more severely felt in some localities than in others. These swells are comparable to those which frequently damage the Atlantic coast of Morocco and prevent the loading and unloading of vessels anchored at open roadsteads. For years these heavy swells have been forecast on the basis of reports of strong persistent winds over the central part of the North Atlantic. We have here examples of local events which occurred as a

result of factors which were operating at a great distance from the place where the events were actually observed.

Diurnal and annual fluctuations may similarly be attributed to factors operating locally or at a distance. These fluctuations are repeated in a regular manner and, by accumulation of data, one can learn how the different factors involved operate, not only towards producing periodic changes but also towards maintaining the steady state. Principally, the methods to be employed are the same within all branches of marine sciences, because in all events we have to take into consideration that we are dealing with a continuous medium.

The periodic events, the annual march of temperature, the annual cycle of the phytoplankton, or the annual arrival of fish to spawning grounds, represent fascinating objects of study, but the cases of abnormal development are often even more fascinating because they may throw light over the problems. Let me again make use of an example. In recent studies of the sardine, which is caught in enormous quantities off the west coast, the question of the occurrence of dominant year-classes has been the object of considerable discussion. The fisheries investigations in northwestern Europe have shown that in certain years the spawning of the herring is very successful and that large quantities of the herring caught over a number of years were born in the same year, say, in 1929. The following years may have added little to the stock, but suddenly a new large group of mature herring of the year-class, say, 1934, may appear. The determination of year-classes is mainly based on the age of the herring, which can be found by counting the rings on the scales or on the otoliths.

Something similar seems to take place here on the west coast although the methods for establishing the existence of year-classes are not so direct. It has been demonstrated, however, that years of successful and unsuccessful spawning exist, and it was recently shown that years of successful spawning coincide with years of high temperature in the coastal waters. Now, years of high temperature are also years of high sea-level on the California coast. It is, of course, possible that high temperature might be advantageous to the spawning of the sardine, but it is not conceivable that the spawning of the sardine should be affected by changes of sea-level, particularly because the differences between succeeding years rarely exceed one or two inches. It appeared, therefore probable that both the temperature and sea-level indicate some other factor which may influence the success of spawning, and it is not difficult to point to such a possible factor. The spawning of the sardine takes place at some distance from the coast, but in order to develop, the young sardine has to be brought in to the coastal waters. High temperature and high sea-level on the coast are characteristic of years in which numerous eddies of offshore water approach the coast, and the success of spawning may, therefore, be related to the type of circulation. The result of the spawning may be good in years during which the character of the current facilitates the transport of the fry and the young sardines from the offshore spawning grounds to the coastal waters. Here we have an example of an event which is not periodic, but for that very reason may lead to the understanding of processes which are in operation every year but which appear conspicuously in a few years only.

This last example also serves to emphasize how closely the many events in our big aquarium are related to each other and how necessary it is to have available information from many branches of marine sciences in order to understand what

happens. It is, however, fortunate that in many instances it is sufficient to examine a small portion of the aquarium, neglecting the fact that all ocean waters are in communication with each other. If we stretch a point we may say that the waters of the Black Sea are in communication with those of the Antarctic Ocean through the narrow straits of the Bosporus and Dardanelles through the Mediterranean Sea, the Strait of Gibraltar, and the North and South Atlantic Oceans, but for all practical purposes the waters of the Black Sea are isolated from the rest of the oceans and the Black Sea represents an independent aquarium. Thus, one can introduce the concept of "degree of isolation" which is very useful when considering the characteristics of any given region. The isolation may be in a horizontal direction due to land barriers or submarine ridges, or it may be in a vertical direction due to the existence of layers within which the density increases so rapidly that no vertical mixing can take place. As an illustration let us consider one of the oyster basins in western Norway where in summer the water-masses in the basin are isolated both horizontally and vertically. These small basins open towards the sea through narrow and shallow channels and in winter are filled by water of relatively high salinity. In the spring a nearly fresh-water top layer is formed by runoff, and this surface layer is of such low density that the deeper water becomes completely isolated because it cannot flow out over the shallow sill, and it does not mix with the much lighter brackish water on top. This isolation leads to characteristic consequences. Part of the solar radiation penetrates through the upper fresh layer and is absorbed in the deeper water in which the temperature increases abnormally because the fresh top layer prevents contact with the atmosphere and thus prevents any loss of heat. The layer of brackish water is often less than six feet thick and may attain in summer a temperature of about 65° Fahrenheit, but by absorption of radiation the temperature below a depth of six feet may be raised to 90° Fahrenheit, and in this warm water oysters which cannot live on the open coast of Norway will thrive and spawn. In winter the deep water is mostly renewed, but if renewal does not take place the oxygen content of the water will be consumed and stagnant water will develop in which no organisms can live except anaerobic bacteria. Such isolated and stagnant deep water without oxygen fills the entire basin of the Black Sea.

The question of the degree of isolation is one which has to be examined when any particular region is selected for intensive study. It is again fortunate that even in the open ocean one need not always take into consideration the entire body of water between the surface and the ocean bottom, because in lower and middle latitudes the circulation within the upper 600 to 800 meters is nearly independent of the deep-water circulation, and a nearly horizontal boundary surface separating two different oceanographic régimes can be established. Vertical boundaries can also be established on oceanographic principles. Coastal waters in middle latitudes are often characterized by lower salinity and greater annual variation of temperature than the oceanic waters in the same latitude. Upon leaving the coast one will mostly observe an increase of the salinity of the surface layers and a decrease of the annual range of temperature, and the boundary of the coastal water can be placed in the region in which no further increase of salinity- or decrease of temperature- range takes place. With due consideration of mixing at this boundary the coastal waters can be dealt with as a separate body of water

which, to a great extent, is under the influence of local factors. In each individual case the oceanographic boundaries to be selected depend upon the character of the region and the problems to be examined, but it must always be borne in mind that introduction of such boundaries also means introduction of boundary conditions which must be fulfilled. As an illustration, let me mention that no net transport of salt can take place through any boundary surface in the sea.

When dealing with the deep-water circulation of the ocean the interdependence of all oceans has to be realized because the deep water of all oceans is formed in the Antarctic and to some extent in the Arctic, and is, therefore, of similar character all over the world. Off the American west coast one does not need to travel five thousand or more miles towards the south in order to find Antarctic water. Antarctic water is much closer because samples of such water can be brought up from depths of about one mile.

A study of the deep-water circulation of the oceans requires data from all seas and necessitates extensive cooperation between institutions of many countries, but otherwise every oceanographic station can select its own region for intensive study and can define the limits of the region according to its general program. Thus, we can subdivide our aquarium and so are not facing the hopeless task of having to examine the entire body of water of the oceans, all of the 330 millions of cubic miles of water, in order to understand all the events in the sea. It is rational to advance step by step and to deal with specific regions and specific questions related to that region in order gradually to build up the complete picture. From this point of view the titles of papers which I quoted as an introduction do not show oceanography as a conglomeration of unrelated subjects, but present on the contrary an example of the detailed work which is necessary in order to build up our knowledge of the oceans. Varied as the list was it bears evidence not of scattered efforts but of the unity of the sciences of the sea.

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MODERN LOGIC AND THE TASK OF THE NATURAL SCIENCES[1]

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1

The task of natural science has been variously conceived. The Aristotelian-mediæval conception that the task is to find the abiding essence of things gave way slowly during the Renaissance to the idea that it is to find the efficient causes of things. This idea still persists in the minds of both many scientists and many philosophers. I shall return to it later. Some scientists and philosophers at the present time, however, would be more disposed to say that instead of finding the causes of things, natural science establishes correlations between things, or rather, between events.

Along with the rise of the idea that the task of natural science is to find causal relations comes the conception that its purpose is to predict and control. This viewpoint reached its classic formulation in the nineteenth century, but it had its beginnings in the Renaissance thinkers who were the fathers of modern science. The Greeks and, to a lesser extent, the mediævals, had a high scorn of putting knowledge into servitude by making use of it in the realm of practical invention. The Renaissance, however, was an age of action. Renaissance people were primarily interested in doing things, and there was no hesitation in putting scientific knowledge to use.

I do not believe, however, that the task of natural science is to predict and control. Instead, it seems to me, that prediction and control are merely verifying principles for the hypotheses of science. They may not be even the most important or the most useful verifying principles, but they are spectacular and thus have a popular appeal that the more sober methods do not have. In any culture, the pursuit of science must justify itself in order to become a widespread occupation, and to an age that glories in action, the ability of science to predict and control is practically a very useful characteristic. But science that has been too eager to apply itself in invention has usually overshot its mark. One of the first persons to formulate the methods of modern natural science, Francis Bacon, himself produced no scientific discoveries of note, and one of the possible reasons for the barrenness of his thought was his eager haste to put science in the service of invention.

There is a difference between pure and applied science. Perhaps it is not an absolute, but only a relative difference. Perhaps at the borderline, pure and applied science merge imperceptibly into each other. Yet the distinction between the two is valid, both analytically and in practice. If this is true, then the task of science cannot be to predict and control, for this would leave pure science with no task.

When it is said that science is concerned with finding cause and effect relations, it is often implicitly assumed that the task of science is to explain phenomena. I am not a little doubtful of this formulation. Of course, the word "explain"

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can be taken in a very innocuous sense, but in this case, I should prefer to use the term "understand," as I shall bring out later. If, on the other hand, we take the word "explain" literally and seriously, then to say that the task of science is to explain is to say that science is more than descriptive. Explanation, in the last analysis, is the attempt to answer the question "Why?" and any complete answer to this question takes us into the realm of philosophy and metaphysics and beyond natural science. It may be doubted that the metaphysical "Why?" is amenable to answer in the same sense as is the "How?" or "What?" of science. If we have metaphysical knowledge, it is a different type of knowledge than is to be found in the natural sciences. Depending on how much of a rationalist you are, you will say either that metaphysical knowledge is the highest type of knowledge, and scientific knowledge is imperfect, fragmentary, partial and thus yields only probability; or you will say that metaphysical knowledge is impossible, only glorified fairy stories, or, at best, hypotheses which, because they go beyond experience for their principles, can never be verified.

The view that the task of science is explanation is very closely bound up, it seems to me, with the view that science is concerned with establishing causes rather than with finding correlations. It is instructive to note that it was the modern philosopher who made the most telling attack on the possibility of metaphysical knowledge who also first defined cause and effect in terms of correlation. I refer to David Hume. Hume's definition of cause and effect has been a bone of contention ever since. Philosophy has not tended to accept it. Often we find those philosophers or philosophies who profess to accept it still arguing endlessly about the question of free will. Obviously, both the traditional and the common sense notion of free will do not make any sense whatever on the basis of Hume's definition that cause and effect is merely an observed invariable succession of events of such a sort that an inescapable habit of mind is set up to expect one event when we find the other.

Some scientists who agree that science is descriptive, nevertheless hold that the notion of cause is a necessary part of science, and that science does more than establish correlations. Max Planck argues at great length that without the notion of cause and effect, different from correlation, natural science becomes merely positivistic, and this means that scientific knowledge is not genuine knowledge, but is only glorified opinion.[6] On the other hand, G. N. Lewis, not only says, just as emphatically, that the notion of cause is not necessary to natural science, but he indicates that, as a matter of fact, science as long as it was scientific never used it anyway. It may have used the word, but it used the logical relation that is much better known as correlation.[5]

The thing G. N. Lewis is inveighing against is the tendency to think that the purpose of natural science is explanation. It seems to me that the history of science is cluttered with explanations (many of them fortunately discarded) that have not only proved useless but that have often obscured the real data of the science. The obvious illustration of the essence from mediæval science is too easy. Not only is that method of science discarded, but Planck would doubtless maintain that his cause is not an explanatory factor in the same sense as the essence. Let me take from psychology an example of the way explanatory factors have obscured the task of a science. Instincts, up to very

recent times, were taken as explanatory factors in psychology. Now, if an instinct is a complex or constellation of tendencies to act so and so in a given situation, very well and good; but if it is a reason *why* one acts so and so, that is not so good. In the first case, one is led to a careful investigation of exactly what the actions and the conditions are; but in the second case, he has found an explanation for the observation with which he started, and there is no reason to go farther. Let me make the illustration a little more specific. If we decide that men fight because of a pugnacious instinct, we are relieved of the immense task of finding the actual factors, economic, cultural, political and what not, that are involved in their fighting. This might strike you as an exaggerated illustration, but, nevertheless, I think it is a good one. The basic physical sciences have pretty well freed themselves of this tendency to take refuge in explanatory factors, but I might suggest that in the so-called social sciences, it still persists strongly, and is one of the main reasons for challenging their true scientific nature.

If the idea of cause is to be kept as a fundamental idea in the natural sciences, then it must be interpreted as a particular type of correlation. The discovery of cause-effect relationships in nature must be conceived as establishing certain types of order in nature.[2] In this case, if you say that the sciences explain events, you do not mean explain in the literal sense in which it answers the question "Why?" You mean it only in the sense of understanding. This leads me to say that a better formulation of the task of the sciences is, not to explain, but to understand. We have understood a situation when we have thoroughly grasped the relations between all its parts. The parts of any given situation may refer outside of itself, and, therefore, understanding also requires grasping the relations between the situation to be understood and others relevant to it. By relevance, I mean the degree to which the parts of the given situation refer beyond itself.

This definition of understanding is summed up in saying that the systematic understanding of a concrete situation consists in the application of some order type to it. The development of order types is the task of logic, and thus the task of the natural sciences is closely bound up with the task of logic. I have already said that understanding, in the strict sense of systematic understanding, depends upon the grasp of relationships. Logic establishes order types by studying relationships. This states the connection between modern logic and the task of the natural sciences, but in order further to elucidate and illustrate it, we must make a brief survey of the growth and fundamental concepts of modern logic.

2

Traditional Aristotelian logic is usually called Formal Logic in the courses of study in American universities, whereas modern logic goes under various names such as Symbolic, Mathematical, Relational Logic, etc. The truth is, of course, that modern logic is formal to as great or even greater degree than is the traditional kind.

Logic is the study of form. The interrelationships between the parts of a thing make up its form. Everything that is complex has form of some sort, because if it has parts, the parts are put together in some way. That which in a literal sense would be formless would be completely fluid: that is, it would

have no discrete parts, or they would be in such continually changing relations that no pattern of change could be identified. Shape is merely a species of form, being the way the parts are put together in spatial relations. Obviously, things, the parts of which are not extended, cannot be put together in spatial relations, but they are put together somehow, and we speak of the form of a piece of music or of an argument.

Form is æsthetic when the relations of which it is made up are apprehended in perception. Form is logical when the relations of which it is made up are apprehended only by intellection. That is, form is æsthetic or logical according to whether the relations are concrete or abstract. It is evident, however, that all I mean by "concrete" is that they are available to sense perception; and by "abstract" I mean "taken out of the context of sense perception."

Logic is the study of abstract form, and its goal is the formulation of abstract order systems. According to the definition handed down from Aristotle, logic is the study of the forms of correct reasoning, and in everyday language, "logical" is synonymous with "rational." The only reasoning developed in Aristotle's time was verbal; hence Aristotelian logic deals almost exclusively with verbal considerations. The development of mathematics in non-verbal symbolism slowly led students to the conclusion that the preoccupation of logic with verbalism was not justified, and in the middle of the nineteenth century, mathematicians began to insist that their subject was as much an instance of logic as was verbal argument. Benjamin Peirce said that mathematics is "the science which draws necessary conclusions."[3]

By the latter part of the century, a fierce battle was raging over the question whether mathematics as a whole could be deduced from the principles of pure logic, or whether, in order to get mathematics, one would need in addition to logical principles certain autonomously mathematical principles. This dispute has advanced far beyond the position that led to the writing of Russell and Whitehead's *Principia Mathematica,* but it cannot be said to be conclusively settled yet. Be that as it may, however, the point I want to bring out is that no matter which side one took on that question in 1900 and no matter which side he takes now, he is, nevertheless, emphasizing the rôle of logical principles in mathematical reasoning; and mathematical reasoning is not strictly verbal.

Thus it occurred to logicians that the best formulation of logical principles would be in mathematical instead of verbal symbols. This could be done by using a non-numerical algebra, now called the algebra of classes, elaborated by Boole in the 1850's. This algebra was improved by other mathematicians and logicians during the latter half of the century, notably by Schröder in 1890. Frege made a calculus of propositions out of it in work published between 1893 and 1903. About the same time, Peano and his group began to formulate all the fields of mathematics in separate postulate sets.[4]

The work of Frege went unnoticed until Russell formulated the logical postulates and theorems of the first part of *Principia Mathematica* on it. The problem was this: What is the relation between mathematics and logic? Peano and his group had formulated a number of mathematical systems. Could it be shown that there was a system of these systems? If so, it would be the principles of logic themselves. Russell and Whitehead started out to formulate

the system of all mathematical systems and thus to settle the dispute by actually deducing mathematics from the principles of pure logic. The deduction is not perfect, but it is so nearly so as to make plausible the argument that the reason it is not is only that logical methods are not sufficiently advanced. We have not yet reached a perfect symbol-system for logic, and results will be inadequate if the symbol system is imperfect.

In order to show how logic deals with relations between terms by means of variable symbols, thus finding general characteristics unhampered by irrelevant concrete connotations, let me take an example. If I say gold is heavier than iron, I am expressing a certain relation between two entities. The same relation may also hold between other entities. Thus, when I say x is heavier than y, I am giving the general pattern of the relation named. But gold and iron may be in other relations also. For example, gold is more malleable than iron. I can say then, gold R iron, thus generalizing the relation between the terms but leaving the terms specific. The complete generalization is given only when I say xRy. The letters in this case are variables of the same sort as mathematical variables. Some specific relation, the value of which is not given, is said to hold between two terms, the value of neither of which is given.

The relation "heavier than" is what we call a dyadic relation. That is, it holds between two terms. But if we start from some such proposition as "New Orleans is between Houston and Mobile," this does not instance the same pattern. "Gold is heavier than Iron" is of the same general form as "Harvard is older than Yale," but "New Orleans is between Houston and Mobile" is of the same general pattern as "John gave a book to Mary." The latter are triadic relations. In order to be significant, they require three terms. Similarly tetradic and, in fact, n-adic relations can be defined; and analogously, a monadic form can be defined, though it would do violence to language to speak of a monadic relation.

This distinction between monadic, dyadic, triadic, etc., forms is an important consideration for some purposes in the study of relations. But let me go on to further distinctions that can be made about dyadic relations.

A dyadic relation is called symmetrical in case it fulfills the following condition: if xRy then yRx. In case it does not fulfill this condition, it may be totally asymmetrical or only non-symmetrical.

A dyadic relation is called transitive in case it fulfills the following condition: if xRy and yRz, then xRz. Again, in case a relation is not transitive it may be wholly intransitive or merely non-transitive.

A dyadic relation is called reflexive in case the following condition holds: xRx is significant.

By reference to these characteristics of symmetry, transitivity and reflexivity, it is possible to make precise distinctions between particular dyadic relations. For example, both mathematics and logic have found it necessary to distinguish between the relation of membership within a class and the relation between two classes wherein one is subsumed under another. Such a distinction is precisely defined by saying that what we mean by the membership relation is intransitive, irreflexive, and asymmetrical; whereas the relation of subsumption is transitive, reflexive, and merely non-symmetrical. In this way we can build up a system of

symbols to denote relations defined with strict accuracy. For example, I have indicated how the membership relation and the relation of subsumption are defined.

There are also other ways of reaching the definitions. For example, if we build up a series of operations between classes roughly analogous to the operations between numbers in arithmetic, we can define relations by means of them and the notion that any subclass derived from given classes by means of these operations may contain no members. Specifically, if we take any two classes, say the class of rich people and the class of happy people, then there is definable what is called the conjunct of these two classes, that is the class of people who are both rich and happy at the same time. There is also definable the disjunct, that is the class of people who are either rich or happy, including those who are both at the same time if there are any. We may also define the subclass of those people who are rich but not happy. This would be the conjunct of the first class and the negate of the second.

Classes may be taken in such a way that any particular subclass does not have members. For example, take the class of odd numbers and the class of even numbers. The conjunct is empty. There are no numbers which are both odd and even at the same time.

Take the class of mammals, for another illustration, and call this class alpha. Call the class of vertebrates beta. In this illustration there are no members of the subclass defined by the conjunct of alpha and non-beta. That is, alpha outside of beta is empty. All alphas are betas, and we say the class alpha is subsumed under the class beta. All mammals are vertebrates. This is another example of the way the relation of subsumption may be defined.

The truth of the matter is, we find that there are always alternate methods of reaching precise definitions, depending upon the place from which you start. One relation is defined in terms of other relations which are more general than it itself. The greater generality is limited in some definite manner, and the limitation affords the definition of the relation of lesser generality. An illustration of this principle may be found by reference again to the membership relation or to the relation of subsumption. A completely general R is first determined to be a dyadic R. Then certain characteristics are ascribed to it which give it a certain value, that is, in this case, the membership relation is a specific value of a general dyadic R. Subsumption is another. These characteristics, which give the special value to the general R, are themselves defined merely by means of the ways other terms and relations go together without reference to their precise values.

This is the way a postulate set is elaborated for any branch of mathematics or logic. Certain terms and relations are taken as primitive, that is, without previous definition. They are put together in such a way as to furnish general principles of a structure. These are the primitive propositions, or postulates, and are assumed without proof. By means of these postulates, other terms and relations can be defined; and theorems can be deduced from different combinations of the definitions and postulates. The process of deduction or proof is the process of explicitly stating what propositions can be shown to be specific

instances of the propositions which are adopted as primitive, and combinations of these.

In this process, it does not exactly matter where you start. Any set of primitive ideas, relations and propositions will give a system in case they are general enough or in case great generality lies in their combination. As it does not matter much just where you start, it is possible to formulate sets that have different primitive ideas and relations and different postulates, but that nevertheless yield the same systematic structure. In these systems, the theorems will be deduced in different order and will have a different dependence on each other. Yet the systems are complete alternatives in the sense that any theorem that can be proved in one can also be proved somewhere in the other. In a similar way, the ideas and relations and postulates that are adopted undefined and unproved in one set will all appear somewhere in the other.

It is also true that two different systems can be set up that are not exact alternatives but which differ from each other in respect to some particular principle. If this difference is slight, there will be many theorems in one which correspond to theorems in the other, but whenever any theorem depends either directly or indirectly upon the changed part, it will be different. Perhaps the best example of the kind of alternatives of which I am now speaking lies in the comparison of Euclidian geometry with Lobachevskian or Riemannian geometries. Hilbert, Veblen, and Huntington have given exactly alternate sets of postulates for Euclidian geometry. Lobachevski altered one part of the system and got a non-Euclidian geometry. Riemann altered the same part another way and got another kind of a non-Euclidian system. All of them are equally perfect logical systems. Any logical system, if fully developed, is the elaboration of an order type. Exact alternatives all yield the same order type. Systems that are not alternatives yield different order types.

3

Now let us go back to the task of the natural sciences. I have said that systematic understanding is the application of some particular order type to the concrete situation to be understood. If one says that the space we live in is Euclidian space, that means he takes our ordinary spatial experiences to be exemplifications of the order system that is elaborated in Euclidian geometry. If he understands our spatial experience, that means he grasps the analysis and structure of relationships we call Euclidian geometry and is successful in applying this logical system to the ordering of experience.

If one says that the space we live in is not Riemannian or Lobachevskian, that means that these order types cannot be successfully applied to our ordinary spatial experience. This statement, however, brings up a very fundamental question as to the nature of scientific understanding and the task of the natural sciences in general. The question is: can a given situation be understood only in one way? My answer is that concrete situations are amenable to understanding in different ways at the same time. It has been challenged, and I think successfully, that the space we live in is Euclidian and not Riemannian or Lobachevskian. In the first place, the theory of relativity uses Riemannian geometry. In our ordinary experience, we deal with space in such small quantities that it

seems to be better described by the order type given in Euclidian geometry. But Einstein finds the Riemannian order type more useful in describing space taken in larger quantities.

Professor G. A. Bliss, of the University of Chicago, has pointed out a way in which all the theorems of plane Euclidian geometry can be interpreted as theorems of spherical geometry.[1] Then, if we take the sphere big enough and the surface of the plane small enough in respect to it, the errors of observation would be greater than the divergence of the two systems. A surveyor of a city lot is quite justified in using plane Euclidian instead of spherical geometry, because his computations are made a great deal easier thereby, and the difference between the applications of the two kinds is smaller than the errors of observation.

It is quite senseless for us to ask whether spatial reality is Euclidian or Riemannian or what else. And it is not part of the task of physics to say whether it is the one or the other. It is the task of physics to find an order system that we can apply fruitfully and successfully to our experiences of space. If our experience with a telescope or a microscope is quite different from our ordinary experience, then it is the business of physics to correlate these different types by finding an order system by means of which they can all be related.

Let me take another illustration. The Ptolemaic and the Copernican theories of the positions and motions of the heavenly bodies are radically different. But I have heard it said by one of the world's leading astronomers that in his opinion the apparent motions of the planets could be as completely understood on the geocentric hypothesis as on the heliocentric; but it would require, he should suppose, somewhere around 85 or 90 epicycles within epicycles to do the job. This would be immensely inconvenient, however, for it would make the mathematics involved in the forecast of an eclipse so complicated that no one would live long enough to complete a single computation.

Professor Bliss somewhat similarly points out that the geocentric hypothesis, modified by a suitable theory of epicycloids, has much to recommend it when we stand out on the lawn and observe the heavens.[1] He says that there is really no advantage to the heliocentric theory except that it is simpler geometrically and mathematically. In the language I have been using, this means that it is understood by reference to a simpler order type. In the light of this, I might ask, does the sun really go around the earth, or does the earth really go around the sun; and my answer is that it is not the business of astronomy to answer this question. The business of astronomy is to find out exactly what the motions of the heavenly bodies are from the point of view of our experience, and then to find an order system that effects the intelligible interrelationship of all these observations. It need not be stressed that astronomy has been highly successful in accomplishing this task.

It has been illustrated that it may be possible to apply different order systems to the same concrete situation. Thus there may be different theoretic understandings of the same situation. One mode of understanding might be preferred to another because it more completely and perfectly subsumes all the observations. For example, the relativity theory in physics is more satisfactory than the Newtonian theory in that it subsumes all the observations the Newtonian

theory does, and in addition takes into account a few things the Newtonian theory does not, such as the observed deviation of the perihelion of Mercury. Few sciences have achieved an order system that subsumes absolutely all observations. There are so-called "residual phenomena" in all the sciences.

Suppose that the two modes of understanding were equally complete in covering any complex situation. The only choice between them then would be based on simplicity. That mode of understanding is to be preferred that is based upon a simpler and more clearly articulated order type. There has been a lot of discussion about the place of the principle of simplicity in scientific method. Its place, according to the view I have been presenting, is that it is merely a methodological principle, but as such is not only justified but necessary. It is a deduction from the purpose of natural science. If the task of science is to understand, and if understanding is the application of a clearly articulated order system to a given situation, then the purposes of understanding are best achieved by that order system in which the articulation is most easily grasped. Man's powers of comprehension are not great, and it is the relatively simpler that is the more easily grasped. The principle of simplicity derives its validity from the fact that it is just another way of stating the conditions of the achievement of the task of science.

Those sciences whose data are subject to quantitative measurement have been most successfully developed because we know little about order systems other than those exemplified in mathematics. We can say with certainty, however, that there are other kinds, for the advance of logic in the last half century has clearly indicated it. We may look for advances in many lines in sciences at present well founded if the advance of logic furnishes adequate knowledge of other order types. We may also look for many subjects of inquiry whose methods are not strictly scientific at the present time to become so when new order systems are available.

I doubt that the advance in science and understanding that I am hopefully forecasting will come primarily from logicians. Instead, I should expect it to come from students advanced in their own fields of study. The rôle for the logician to play is to work out the knowledge of abstract order systems. If that knowledge is available, some day along will come the student of a special field of investigation who will be acquainted with the knowledge of order systems, and he will have the insight necessary to apply such knowledge to his specialty. This has frequently happened in the case of the connection between mathematics and the natural sciences. Mathematicians have often formulated mathematical systems without the slightest idea of any possible application; and then, sometimes generations later, the use of this abstract mathematical system in promoting understanding of concrete experience has become apparent. Therefore, the important thing for logicians to do at the present time is to plug away at the task of developing logic; and the thing for the other specialists to do is to plug away at the attempt to understand their own specialties. It is desirable more and more, however, that the other specialists should be acquainted with logic, because the farther logic is developed and the more the other specialists are acquainted with it, the greater will be the probability that useful applications will be discovered and understanding forwarded.

(*Please turn to page 139*)

THE FORTY-FIRST ANNUAL CONVENTION

The 41st annual Convention of the Society of the Sigma Xi is scheduled for 4.00 P.M. Monday, December 30, at the Bellevue-Stratford Hotel, Philadelphia, Pennsylvania. Among the important items of business which the Convention will be called upon to consider are the following:

a. Petitions for charters for the establishment of chapters at Bryn Mawr College and Oberlin College.

b. Report of Committee on Membership Structure.

c. Reports of the President, the Secretary, and the Treasurer.

d. Election of a member of the Executive Committee and a member of the Alumni Committee for a five-year term.

The Nominating Committee this year is composed of Professor P. H. Mitchell, Brown University, Chairman; Professor F. M. Carpenter, Harvard University; and Professor Ernest Carroll Faust of Tulane University.

Chapters may make suggestions to the Committee direct, or through the National Secretary.

e. The 19th annual Sigma Xi lecture will be given Monday evening, December 30, by Dr. A. J. Carlson of the University of Chicago, on "Science versus Life."

INSTALLATION—UNIVERSITY OF SOUTHERN CALIFORNIA CHAPTER

The installation ceremonies of the University of Southern California Chapter of the Society of the Sigma Xi were conducted on May 24 by Professor George A. Baitsell, National Secretary, assisted by Professor Carl D. Anderson, a member of the National Executive Committee. The initiation and installation proper were preceded by an All-University Convocation at 11.00 A.M., followed by a luncheon in honor of the assembled delegates, guests, and charter members. Impressive and dignified installation exercises were conducted at 3.00 P.M., presided over by Professor Baitsell, chief installing officer, assisted by Dr. Anderson.

A large and responsive group of delegates from the various chapters, clubs, and scientific societies graced the occasion with their presence, and words of felicitations and good wishes were received from chapters and clubs not sending delegates.

Following the dinner at 6.30 P.M., the evening science lecture on "The Eclipses of the Sun" was delivered by Dr. Dinsmore Alter, director of the Griffith Park Observatory, Los Angeles.

RECENT CONTRIBUTIONS TO THE PHYSIOLOGY OF BIRTH [1]

DONALD H. BARRON
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Fifty-five years ago there appeared from the pen of Wilhelm Preyer, professor of physiology at the University of Jena, a book entitled *Speciele Physiologie Des Embryo* and sub-titled *Untersuchungen Ueber Die Lebenserscheinungen Vor Der Geburt.* A volume of 652 citations, it organized the established knowledge of the day in that field, together with the essential historical background. The book was well received by those to whom it was addressed, and a French edition appeared the following year.

The introduction to the book contains material that leaves no doubt about the author's purpose in its publication; he pointed out that, whereas the morphological development of organs and organ systems had been followed systematically, no such observations were available on physiological development and embryology, and that just such a biochemical and physiological embryology was essential to the understanding of the functions of the newborn. The book was intended to focus interest on the subject and to form the basis for systematic studies of the functional aspects of embryonic growth. In the year of the French edition it seemed likely to succeed in its purpose, but interest in this branch of embryology waned as it waxed in morphogenesis, and the knowledge of the physiology of the embryo contributed in succeeding years was largely based on deduction from structure and not upon direct experiment. The reasons for the decline of interest are doubtless many. Anatomy had advanced at that time to a point at which morphogenesis had become a useful tool in the solution of its problems, whilst physiology, on the other hand, was itself in the embryonic stage; the functions of the adult were to receive first attention as had adult structure. Today, the physiology of the adult appears to have reached a stage in which the study of the functions of the developing embryo can be pursued with profit, so that after fifty years the tables have been turned; physiological embryology has aroused new interest in Preyer's book.

This present-day interest in the physiology, as distinct from the biochemistry, of the embryo stems from two roots. The first was provided by Coghill in this country, who studied the development of motility in amphibian embryos as an approach to problems of behavior. The second is associated with Sir Joseph Barcroft and his school at Cambridge. Through an interest in the adjustments of the maternal blood vascular system that provide for the extra burdens upon it during pregnancy, they were led to a consideration of the blood and circulation of the fœtus. Both of these lines—the study of the development of behavior and circulation—have been expanded until they were joined in Barcroft's Terry Lectures at Yale in 1937 which dealt with the interrelations of the developing nervous and blood vascular systems.

Space does not permit a consideration of more than a small part of the information already accumulated in this new branch of embryology, and I have chosen,

[1] This article contains the substance of a lecture given at the Biological Laboratory, Cold Spring Harbor, L. I., 1940.

therefore, a piece which appears to cut across a great deal of that knowledge, and one with which everyone has had first-hand experience, the physiological adjustments in animals at birth.

When an animal is born and begins its struggle toward an independent existence its first efforts are those of breathing. Breathing is living; the onset of respiration is the beginning of life. But the newborn animal does not attain life easily; the breathing efforts are labored, and the chest heaves with each gasp until the lungs are filled with air and regular respiration is established. As life so directly begins or ends with the establishment of respiration immediately after birth, we may inquire what precautions nature has taken to insure that the first breath is not the last. Has anything in the nature of a rehearsal taken place before birth?

If the sheep embryo is typical in this respect, the answer is yes. Studies of the development of the behavior of the sheep embryo, after the manner employed by Coghill on amphibia and Windle on mammals, have revealed that the muscles of respiration are among the very first to be brought under the control of the embryonic nervous system. These muscles work together after the fashion seen in the adult, so that they would expand the chest if its bony frame were rigid. Such a synchronized effort of these muscles occurs in young embryos only when the nose is stimulated. Each time the nose is touched the respiratory muscles contract. Not until later in development do these contractions follow one another at regular intervals as they do in the adult, and even then the rhythmic series lasts only as long as the fœtus is moving about; however, in this stage it is no longer necessary to touch the nose, so that any stimulus that arouses the whole fœtus is effective. There is at this stage in development a definite dependence upon muscular activity. The next advance can be seen in older fœtuses. The respiratory muscles in them contract rhythmically and quite independently of other muscular movements. Such an advance insures the continuation of respiration in quiet states as sleep. *In utero,* of course, these movements never bring air into the lungs, for the fœtus is surrounded by fluid. Even if they result in this fluid being drawn into the lungs no harm results, for the fœtus derives its oxygen *in utero* through the placenta from the blood of the mother. Further development results in a definite suppression of all activity, respiration included. The fœtus remains inert until birth, with the capacity for respiratory movements established but latent.

By appropriate surgical attacks upon the fœtus *in utero* that do not interfere with subsequent development (a technique first introduced by Nicholas at Yale) these succeeding stages in the establishment of the ability to breathe regularly, can be shown to be related to the longitudinal development of the brain. Each advance in the respiratory mechanism is made possible by the onset of function in a new portion of the brain. Oddly enough these parts of brain appear to be arranged in serial order, one ahead of the other along the brain. The brain center essential for the first organized contraction of the respiratory muscles is in the end of the medulla oblongata nearest the spinal cord, whilst the rhythmic character is added by the development of regions in the opposite end near the point where the auditory nerve enters it. The dissociation of the breathing rhythm from other muscular movements is made possible by the development of

cells in the midbrain just ahead of the entrance of the auditory nerve. The region of the brain above this dissociation center, the cerebrum, is responsible for the inhibition of activity that lasts until birth.

By surgical removal of these parts of the brain from above downwards in a fœtus whose breathing *in utero* is in the inhibited state, the central respiratory mechanism can be resolved step by step into its simpler components. A similar resolution can be effected if the fœtus is asphyxiated; a simple way of doing this is to prevent the circulation of the fœtal blood through the placenta, the organ in which oxygen is transferred from the maternal to fœtal blood. If this can be done the fœtus arouses itself from inactivity and respiration begins. As asphyxiation progresses the breathing is accompanied by general activity. This stage gives way to one in which the fœtus resembles an animal gasping for breath and finally the rhythmic character is lost. At last, the muscles only contract when the fœtus is stimulated.

This simple experiment led to the assumption that those parts of the brain, concerned in respiration, which developed first were able to function at degrees of asphyxiation at which the new parts could not. The assumption has found support in careful studies by Himwich and his colleagues at Albany, who have shown that the first parts of the brain to develop have the lowest metabolic rate, the last to develop the highest. Though asphyxiation renders these parts of the brain non-functional, unless prolonged it does not kill them, for the embryonic brain, even the delicate cerebral cortex can survive long periods of asphyxiation, as Kabat of Minnesota has demonstrated on puppies.

The demonstration of this differential susceptibility of parts of the brain to asphyxia and the resultant release of the older patterns of respiratory activity is made by almost every animal born in the normal way. The studies on the blood gases of the fœtal sheep carried out by Sir Joseph Barcroft and his collaborators have shown that as pregnancy nears its end the amount of oxygen in the fœtal blood declines. This is particularly true during the last ten days. The amount of oxygen available appears usually to be sufficient to maintain the centers of the brain which inhibit respiration but the margin is a small one, so in some cases it happens that the inhibitory center is unable to function and quiet respiration begins *in utero*. Once labor has begun the fœtus is deprived still more of its oxygen supply and finally, when the delivery has been accomplished, the asphyxiation is even more complete; but unless it reaches a degree that prevents the very lowest center from functioning, gasping efforts occur that bring air into the lungs. The more pronounced the asphyxiation, within limits, the more violent the respiratory efforts.

Gradually the breathing efforts of the newborn animal raise the amount of oxygen in the blood from the low level reached when the placental circulation was impaired—30 to 40 p.c. saturated—to about 75 to 80 p.c. This may be accomplished in some cases in as little as three minutes. It may, and often does, take longer. Now you will remember that *in utero* the fœtus was quiet when the blood was saturated to the same extent with oxygen, and it might, therefore, be expected to cease breathing and become inactive until the oxygen in the blood fell again. We know, however, that the blood in the adult is richer in oxygen, for the percentage saturation is about 90. How does the lamb manage to keep

actively respiring even when its blood is oxygenated sufficiently to bring into play the cortical inhibitory activity?

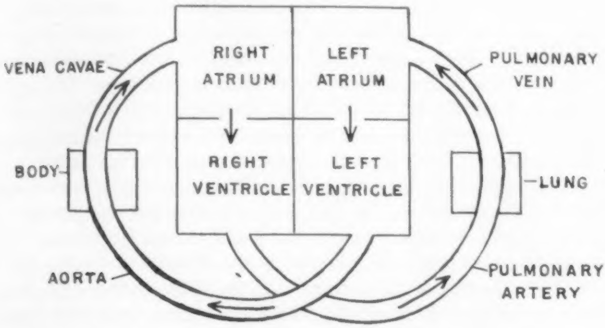
The answer to this question can only partially be given at this stage, but the direction in which it must be sought seems clear. The depression responsible for the inactivation *in utero* can be overcome if the fœtus is stimulated violently enough. In other words, the inhibiting effect of the cerebrum on the general activity of the fœtus, including respiration, can be overcome if sensory impulses arrive in the nervous system in sufficient numbers. The fœtus will gasp in response to stimuli applied, for example, to the sciatic nerve, but *in utero*, as the fœtus is not exposed to stimuli that can affect any sizable portion of its sensory nerves, the inhibitory center is in control. Once the fœtus is in the open air its sensory system is activated by changes in temperature and contact with objects, and, as the muscles begin to take on tone, impulses arise from the endings within them and at the joints. All this sensory activity appears to counterbalance the inhibitory effects of the cerebrum when the oxygen in the blood returns to the intra-uterine level. As a result, the respiratory efforts are continued and as the ventilation of the lungs improves the blood eventually reaches an oxygen saturation of the adult level—90 p.c. This may take a matter of hours, though it can be accomplished in less.

The blood that circulates in the newborn body is, therefore, a good bit richer in oxygen than it ever was *in utero*. Under these conditions, the brain expresses new potentialities, potentialities that were inhibited *in utero* because of the poor oxygen supply, and inadequate sensory information. The balance appears to be a delicate one *in utero*. An increase in the oxygen in the blood would bring increased fœtal activity, by enabling new centers to function. A decrease in the oxygen supply would result in increased activity by rendering certain inhibitory centers non-functional. Normally Nature seems to take a middle course, one that results in an inactive fœtus, a fortunate circumstance for the mother.

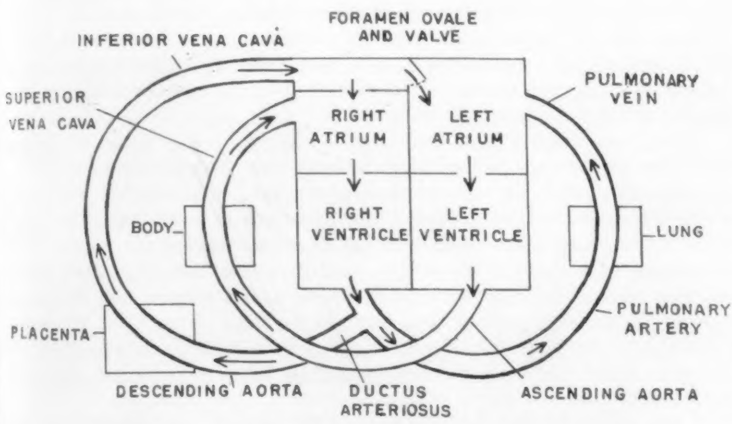
Thus far we have considered only the establishment of lung ventilation at birth. From the lungs the oxygen must be carried to the tissues by the blood stream. We may, therefore, ask what alterations are made in the vascular system to balance the change in the organ in which the blood is oxygenated, the substitution of the lungs for the placenta.

X-ray cinematograph records made at the Nuffield Institute in Oxford by Barclay and Franklin have demonstrated that the rate of blood flow through the lung before birth is very, very slow indeed. The rate may increase slightly during the last week of gestation, but rises enormously as the breathing efforts of the fœtus succeed in bringing air into the lung. The mechanisms that exist for shunting the blood away from the fœtal lung are revealed by a comparison of the adult heart with the fœtal heart. The adult heart pumps blood through two separate circuits. The auricle and ventricle of the right side receive blood from the body and pump it through the lungs; the corresponding two chambers of the left side receive blood from the lungs and pump it through the body. In the first circulation the blood is oxygenated by the lung; by the second, the oxygen is brought to the tissues. The two circulations are quite separate. A similar arrangement exists in the fœtus, though the line between these two circulations is

not so sharply drawn. The two circulation patterns—adult and fœtal—are schematically represented in Figure 1. A comparison of the adult with the fœtal



(a) Adult circulation.



(b) Fœtal circulation.

FIG. 1. Diagrams illustrating the circulation of blood through (a) the adult and (b) the fœtal heart.

scheme reveals the chief difference to be the placental circulation of the fœtus. Though this circulation is not accurately portrayed, the diagrams allow for a consideration of the principal changes at birth as stripped of their detail.

Before birth, as indicated above, the blood flow through the lung is negligible. The pressure developed by the right ventricle, 70-80 mm. Hg, does not appear to be sufficient to force blood through the uinflated lung. The resistance is higher in the lung circulation than in the placental, so the blood is forced along the path of least resistance. The gradual inflation of the lung is accompanied by a

steady decline in the resistance in the lung circuit, and more blood enters the left atrium along this route. Hand in hand with this decrease the resistance in the placental circuit is increased by a gradual narrowing of the ductus arteriosus (Figure 1, b). This ductus is a portion of the placental circulation; unlike the other parts of the system it is built like a sphincter and is under the control of the nervous system. The center in the brain that controls the ductus appears to time the closure of the sphincter with the gradual expansion of the lungs.

The blood reaching the left atrium from the placenta before birth did so at a pressure between 20 and 30 mm. Hg. With the increased resistance in the circuit, the pressure drops towards zero. At the same time the pressure and rate of flow into the left atrium increase with the result that the check valve in the foramen ovale is closed and no blood is lost backward into the placental circulation which finally fails completely when the umbilical cord is severed.

This cycle of events has been followed in fœtal lambs delivered by cæsarian section. The order in the sheep is as follows: the lung circulation opens up with the onset of respiration, the ductus gradually closes, and finally the valve of the foramen ovale is closed by the increased flow into the left atrium from the lung circuit. All this can and does happen normally within ten minutes after the fœtus has been delivered so that the physiological change in the vascular system is an abrupt one. The anatomical obliteration of structures concerned in the placental circulation, as morphologists have shown, proceeds after this physiological change in a leisurely fashion to be completed in the course of two or three months.

We might profitably pause here to survey briefly another aspect of the fœtal circulation as revealed by the X-ray techniques of Barclay and Franklin. It relates to the right atrium. In the figure of the fœtal circulation the right atrium is diagrammed as being composed of two parts, one of which—the continuation of the inferior vena cava—opens into the left atrium through the foramen ovale. An opening in the side of the inferior vena cava leads into the second division that also receives the superior vena cava. In such a scheme, and this scheme portrays the essential facts as they occur in the fœtal sheep heart, the streams of blood from the body and placenta can be kept quite separate except for such mixing as might take place through the opening between the inferior vena cava and the right atrium.

It was generally held until the turn of the century that these two streams are kept separate and that as a consequence the blood from the placenta, rich in oxygen, was sent to the body, particularly to the head, whereas the blood poor in oxygen was directed back into the lower part of the body and the placenta. Those who have opposed this view have concluded that the two streams are not separated in the right atrium and that as a result a complete mixing of the body blood streams takes place. In circumstances such as these, the oxygen in the blood going to the head would be identical with that in the blood destined for the body and the placenta.

X-ray studies of the circulation of the fœtal sheep show quite clearly that when radiopaque substances are introduced into the blood of the superior vena cava the shadow always outlines the right atrium and ventricle but no shadow appears in the left atrium. The blood poor in oxygen, upon its return from the

head and arms, is diverted directly into the placental circulation. On the other hand, when the radiopaque substances enter the fœtal heart along the inferior vena cava the shadow may be confined to the left atrium. Not enough of the substance is diverted into the right side of the heart to form a discernible shadow on the film. Toward the end of pregnancy such a shadow does appear. As birth is approached more of the contrast material is diverted into the placental circulation. We may safely conclude that the oxygenated blood from the placenta is destined for the left atrium en route to the head and the body. Some of it may be lost on the way and passes into the right atrium from the inferior vena cava. Nature has arranged to pay a price in good blood to insure that the main volume carried toward the head is relatively uncontaminated. Thus the old ideas about the circulation have been substantiated by the modern techniques.

A great deal more has been learned by these X-ray methods; we have touched only the highlights. More might be said about the changes in the blood vascular system at birth. Those we have considered take place quickly though slower changes do occur. For example, the hæmoglobin—the oxygen-carrying pigment in the blood—is not the same in fœtal and adult animals. The fœtal hæmoglobin has a greater affinity for oxygen at the same partial pressure than has the adult type. A similar difference has been found by Professor Hall of Duke University between the hæmoglobin of tadpoles and frogs, the unhatched chick and the chicken. The conversion from the fœtal to the adult type begins toward the end of gestation or incubation period and continues for some time afterward. About this change we know very little as yet but the problem is being actively investigated.

Did space permit we might go farther afield, but my purpose has really been the purpose of Preyer, to arouse your interest in fœtal physiology, the problems of systematic integration it presents, and to emphasize his thesis: a knowledge of these problems is essential to an understanding of the physiology of the newborn.

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Modern Logic and the Task of the Natural Sciences

(Concluded from page 125)

The understanding of any concrete situation is the application of a thoroughly articulated order system to it. Therefore, the greater our knowledge of order systems, the more promise there is of the understanding of concrete experience. This is what I conceive to be the connection between modern logic and the natural sciences.

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...stitute	M. A. Countryman	B. S. Radcliffe	L. T. Anderson	D. L. N. Hedrick
...pi	F. M. Hull	A. B. Lewis	G. W. Parsons	G. W. Parsons
[illegible]	C. E. Braun	P. A. Moody	H. B. Pierce	H. B. Pierce
...te Agr. Coll.	L. B. Linford	G. F. Knowlton	L. E. Malm	L. E. Malm
... Young ...sity	W. B. Hales	B. Harrison	A. J. Johanson	A. J. Johanson
...ollege	C. Welty	....	N. Bennington	N. Bennington
... Poly. Inst.	B. T. Simms	R. Allen	H. R. Albrecht	H. R. Albrecht
... University	O. R. Quayle	J. H. Purks	E. Papageorge	W. B. Redmond
... State College.	Z. P. Metcalf	W. H. Browne	R. C. Bullock	R. C. Bullock
[illegible]	C. J. Hamre	M. Potgieter	S. S. Ballard	S. S. Ballard
...pi State ...	C. H. Ragland	F. P. Welch	J. F. Locke	J. F. Locke
... Nevada	W. D. Billings	I. J. Sandorf	L. R. Williams	L. R. Williams

OFFICIAL INFORMATION

Sigma Xi Insignia

All insignia of the Society are available, upon prepaid order, through the office of the National Secretary only. Information about styles and prices may be obtained from Chapter Secretaries or the National Secretary.

Diplomas for Members and Associates

Engrossed diplomas for members and associates are available at 35 cents each. Since the diplomas must be signed by the President and Secretary of the Chapter in which the individual was initiated, orders should be sent direct to the Chapter Secretary.

Constitution and History

Printed copies of the "Constitution and Historv of the Society" may be obtained from the National Secretary in any quantity at 5 cents each.

Sigma Xi Half Century History and Record

Copies of the "Half Century History and Record of Sigma Xi" are available at $3.00 each upon prepaid order to the National Secretary. This volume was published in 1936, and contains an alphabetical list of members and associates initiated into the Society, and a brief history of the organization of Chapters and Clubs to that date.

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